

Social Research in East Africa; Varying Opportunities and Methodologies. A Personal Approach

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INTRODUCTION

The purpose of this study is to show the importance of four factors in anthropological research carried out by one individual¹, opportunity, luck, training and enthusiasm resulting in some hundred publications using a variety of research methods in which the data constrained the methodology rather than the methodology predominating. These four factors possibly combined to show the disadvantages of commitment to a particular specialised line of research in either my careers as a civil servant or as an academic.

1. OPPORTUNITY THE STRUCTURAL BACKGROUND

I started research with a number of accidental advantages. Firstly being bilingual as the Tanganyika Administrative Service required that all officers had to pass the written and oral Higher Swahili Examination. This was not just written learning as Swahili was the lingua franca of Tanganyika from the time of the German suzerainty and used by all those in the government administrative service and most Tanganyikan men but not many women, in their dealings with government, education and commerce. This idiomatic knowledge of kiSwahili used all day and every day which the writer ultimately used as the medium for university lecturing on politics and industrial relations in Kenya, meant that in the research which preoccupied me latter, there was no long period spent in learning the language (Tanner, 1996), which preoccupies external as well as possibly some internal expatriate anthropologists when they first start field research.

Secondly the writer had very little understanding of the communities in which I was an administrator. I had assumed that everyone everywhere were basically the same, since my prior training for six weeks as a probationary administrator was in accountancy, office administration and law. Thus any fact that came to my administrative notice such as the role of

the mother's brother in the education of children among the matrilineal Yao of Tunduru, had to be coped with anthropologically or in other words by learning from those involved and not as some form of bizarre behaviour which had to be controlled.

Thirdly I was posted to a locality by government order, often being notified only a few days before return from leave and having to stay there until moved with three year tours. My house and transport were paid for by the Burma and Tanganyika Governments under strict budgetary controls and also administrators were required to tour their areas for ten days every month. In effect we were required to do field work, looking for matters of general concern; lack of food reserves, erosion, illness (I once came across a smallpox case when that disease was thought to be nationally extinct), illegal drinking stills, corruption, tax default and local government inefficiency or officials coming to be regarded as not fully competent. As the writer did most of this touring on foot, rather than by car, it was virtually impossible to be unaware of the social environments through which I was walking.

Fourthly personal characteristics are important and are not often considered in the life-time of anthropologists. The ability to attract informants and to cope with loneliness as well as an energetic inoffensive curiosity. The writer appeared to be a workaholic and this fitted in well with systems methodologies and as an extrovert I did not mind non-participant observation which fitted in with action methodologies.

Finally the time in north Burma among isolated tribal peoples meant that the excitements and officials coming emotions associated with cultural strangeness were over and done with and effectively cut off by the move to Africa. I was thus able to approach research in a much more detached manner and did not appear to have felt 'proprietary' rights over any place, people or purpose, since I had no choice over the area and community to which I was posted; for instance having got a degree for a study of a Moslem coastal society, I was posted to the west of Tanganyika on the Ruanda border where there

were very few Moslems.

This dialectical situation (Geertz, 1988, 70) forced on the writer by situations over which I had no control and indeed little personal inclination to attempt to control, prevented the mistake of pretending that I could become even a small part of the host cultures such as the illusion of getting oneself initiated into some indigenous groupings. This otherness insured my isolation within my own recognisable cultural biases such as the revulsion which I felt at animal sacrifices and my dislike of public drunkenness as opposed to seasonal inebriation.

Wherever I was posted there seemed to be very little I could read about the peoples with which I was involved administratively. In my first station at Kilwa there was some archaeological data but nothing more. Thus it struck me at once and indeed continued to do so over the years even when I returned to Britain, that whatever I enquired into, I would probably be the first person to do so. There was rarely the need to look into other people's field work on the topic because there was none.

This meant that I experienced by the accident of administrative postings the tribally decayed Ngoni of Songea, the matrilineal Yao of Tunduru, the hierarchical Hangaza of Ngara, the coastal Swahili of Pangani and Kilwa and the pastoral farming Sukuma near Lake Victoria, a broad anthropological foundation which prevented emotional involvements or specialisation.

Personal encouragement is difficult to evaluate but this must have been important. The Tanganyika Government sent me to Oxford for a year for administrative studies during which I got their Diploma in Social Anthropology. I remember that the factual lectures were of value as I could tick off the contents against what I knew from my previous contacts with these tribal cultures; for example I had already some 'practical' experience of the gumsa-gumlao situation among the Kachins (Leach, 1954). Professor Evans-Pritchard arranged for me to take a first degree by thesis on the social system of the coastal Swahili. However Dr D. Roberts latter Professor of Genetics at the University of Newcastle, saw that I was a 'cheap' in residence overseas source of data worth cultivating and organising for which grants did not have to be applied for. There are important advantages in being a satellite to a more centrally placed and important academic in which it may pay not to be too ambitious.

Both recognised that officials working

overseas were field work assets. The former in the long lasting sense of accumulating data about societies which were rapidly changing. While the latter saw me as an inexpensive source of data and cooperation; he worked up my colour blindness (Roberts and Tanner, 1967) and infertility surveys (Roberts and Tanner, 1959). All in all I had fortunate foundations for all the research which I subsequently undertook.

2. LUCK THE FORTUITOUS NATURE OF RESEARCH OPPORTUNITIES

Looking back on a long series of research projects it is hard to avoid the conclusion that almost every one was the result of some accidental opportunity. Only once did I plan in England to do a particular piece of research, a fishing community when I heard that I was to be posted to a coastal district, only to find that no one there lived exclusively by fishing.

The distribution of rock paintings in Usukuma because I accidentally found a totally unrecorded series when walking (Tanner, 1953) and the sources of Chinese pottery in Kilwa (Tanner, 1952) and Pangani because I found quantities on the beaches and cliffs. I noticed that there were more Moslem than Hindu defective children in Mombasa so I studied the cousin marriages of the entire Afro-Arab community there (Tanner, 1958, 1964).

As an Extra-Mural Tutor in Mombasa I had offered to teach in the prison which developed into a two year study of convict behaviour (Tanner, 1969, 1970, 1972). I had no previous experience or knowledge of criminology. As a result of writing a cabinet paper on police manning levels in Uganda which showed that as the ratio of police to people expanded, reported crime rates would go down, the Commissioner of Police asked in what ways he could help its writer. I asked for the murder files for 1964 (Tanner, 1970), and I developed this contact further for work on criminal statistics (Tanner, 1970).

When 10% compulsory annual destocking was enforced in Usukuma, Tanganyika and there were no riots, the Provincial Commissioner asked me to find out why. This research into the uses of livestock showed that stock owners were either slaughtering, selling, loaning or giving out for bridewealth more than the required 10%.

Academics and others stuck in their own countries by teaching commitments and shortages of research funds recognised that I was an asset which did not require funding.

Hence my research into diet patterns, using a method of quality assessment rather than suspect quantities (Tanner, 1956), colourblindness, which showed that even using the Ishihara test awareness of colours depended in part on education, infertility and inbreeding. I collected medicinal plants for the British National Herbarium at Kew, Loma Linda University in California and for the Wellcome Foundation in London. In ten years my wife and I collected over 6000 plants with their local names and uses. This interest in plants led to countless discussions with knowledgeable men from which other understandings developed.

Then there were repeated moments of plain opportunism. Held up by floods I asked some watching children to demonstrate their games. While sheltering from rain in a house the owner worked out his reciprocal obligations according to his family tree showing the ins and outs over the previous year which in fact were almost exact equivalents. It seemed essential to snatch at any opportunity rather than wait for them to be provided for you or have them restricted to narrower previously defined research objectives. I was often surprised at the physical inactivity of professional researchers funded by grants.

3. ENTHUSIASM THE IMPULSE FOR TIME CONSUMING RESEARCH

Why do research at all? I could have coped with the mother's brother or whatever administratively and tactfully without needing to go any further. The impulse to do research extensively came from multiple sources.

My wife once remarked that all I saw on my frequent tours was the toes of my boots and that I was learning nothing in this process. As a result of this I started in Kilwa to record useful trees and their distribution (Tanner, 1951).

Life as an administrative officer was always culturally isolating. In any station there were few Europeans with whom to associate and even fewer with whom I would have wanted to associate, apart from role conflict difficulties as the senior official and magistrate in a district.

This meant three years in the same place with the same people which could be and often was stupefying; one ran out of conversation and what conversation existed was anecdotal and personalised. I subsequently researched this type of 'tribal' life (Tanner, 1964, 1966); discomfort need not be part of field work.

Hans Cory the Government Anthropologist in Tanganyika warned me early in my service to avoid what he called the two great African temptations; drink and women. So a spare-time filler was needed since neither my wife, an artist and poet nor myself had any interest in costly drinking and segregated clubs coming as we had from Burma where they no longer existed.

Some years into his Tanganyika service I was faced with a personality clash which meant that unless I diverted myself into some activities within the orbit of government work, I would inevitably have personal and professional problems. Thus the only way out of this cultural desert and professional road-block was to turn outwards to the vibrant society that was surrounding me, and about which I knew little outside the requirements of legal work and local government administration. So not only were there fortuitous elements in the choice of research topics but the push of particular personal circumstances.

The atmosphere in which research is undertaken fails to record more human factors which certainly are involved; enmity and rivalry. Without doubt the fact that I was virtually uneducated professionally led me to question what the educated were pronouncing on about the societies in which I had by accident better or at least more intimate knowledge through my language expertise and official work. It may no longer be quite 'my tribe' but 'my box' of knowledge which professional researchers were invading whose conclusions I was tempted to question.

This occurred initially with Hans Cory whose Sukuma Customary Law in Swahili translation had been provided for the use of every local court (Cory, 1953). I found that its pronouncements which had all been recorded with the concurrence of chiefs and elders, did not correspond with what I was coming across in dealing with appeals from local courts. So I researched the ownership of land and the installation rituals of chiefs (Tanner, 1955, 1958, 1960) to account for these differences.

As an administrator faced with daily conversations with people with problems I found the interests of the anthropologists whom I had met and listened to, were often detached and unrelated to the realities of life of those whom they had been studying and with whom they did not have to live, and whose problems they did not have to attempt to solve. This I found distressing. It seemed to me that they were facing in-

wards towards their own careers rather than outwards, a theoretical rather than an applied approach. It was academia talking to itself. I wanted information that I could use sooner rather than latter to help me understand what was going on.

Finally competitiveness. When I did get to Oxford and listened to lectures and seminars, it struck me very forcibly that I was often listening to a great deal of thought based on a very limited amount of field work. I felt ingenuously that I could do better and more quickly than that with the opportunities given to me. When I was Ford Foundation supported Chairman of the East African Institute of Social Research in Kampala I found that in that three years, one third of the grant supported academics visiting Uganda for research, wrote nothing at all; they were attenuated tourists. I found this to be a serious and uncontrolled waste of public money.

4. TRAINING BECOMING A PRACTICAL ANTHROPOLOGIST

Plant collecting was an invaluable lead into anthropology. Plants have names and names have uses. I found that my office orderly Juma Ndamulo alone could name more than 500 Sukuma plants. Plants led to medicine and illness and illness led to metaphysical issues quite outside an Englishman's understanding. This led to the collecting of rare words and the awareness that many Africans did not see the same colours that he was seeing, blue, purple and orange were absent from the Sukuma's vocabulary. I was also stimulated from the fact that by collecting so many plant specimens like a vacuum cleaner, I was finding new species.

So it seemed to me that I had to learn something of Sukuma society in a systematic rather than gossipy way. I borrowed Evans-Pritchard's *Azande* (Evans-Pritchard, 1937) from the Government Anthropologist because of the title, putting each word in the index on the top of blank sheets of paper, and started to ask the tolerant Sukuma whether they did what the Azande did and if not why not.

This may seem simplistic but I appeared to have given myself a basic course in anthropology via this one book with the support of a couple of dozen Sukuma who thought it was to their advantage as well as interesting to spend hours talking to a government official about what they did all the time. This led to a series of publications (Tanner, 1956 a, b, 1957 a, b, 1958, 1959).

Several particular men stay in my memory as

being exceptionally alert to the intricacies of their own culture; traditional healer Kabadi, my orderly Juma Ndamulo and Chief Kapongo of Nassa. The return which I gave them for this expenditure of their time and tolerance cannot have been much more than hospitality. I can only conclude that they and many others over the years must have enjoyed these seemingly unending dialogues since one answer always led to another question. Much of this was subsequently recorded on long playing tapes. Certainly I used informants ruthlessly and discarded them with the passage of time and on leaving the area but I think that neither side deluded themselves into thinking of friendship; a much used word in the introductions to anthropological publications, and in fact these relationships mirrored the brevity of most so-called friendships with European colleagues.

5. THE PASSAGE OF TIME AND THE URGENCY OF RESEARCH

It has always struck me that if the object of field research is service to the community and to those involved with that community, then quickness and cost cutting is more than justified. I admit that this is simplistic but in practical terms simplicity may be nearer to reality; data addiction to scientific methodology may be one of the delusions of the social sciences.

It should not be a question of how long can this research be spun out for but how soon can a reasonable job be done and a readable report written. While I was Chairman of the East African Institute of Social Research, educational researchers produced a detailed report on Ugandan secondary schooling for the Ministry of Education. It was presented to them with a suitable exchange of compliments, I later found that the pages had been misbound and thus the report made no sense at all. No one commented on this or on the findings since only the two page summary had been read.

So many researchers seemed to be writing in mental isolation and for an audience in another country. This meant that in many matters they did not see the wood for the trees as for instance education is not necessarily a 'good thing' when it is interlocked with drought, intestinal parasites, inflation, civil unrest and corruption.

I found that there were few issues on which it was not possible to reach usable conclusions within a three month field research deadline and three months writing up. Perhaps researchers if

they are not paying for their own field work, should be made to justify the extensions of their grants on a month to month basis or by some such arbitrary deadline. Too often it seemed to him that academic freedom was a cover for idleness.

There appear to be two functions of research for which academic standards should dominate. One is the question of legal liability which came up in my study of prison behaviour and possible danger to those involved and that high quality staff need only be used when that high quality is necessary in hard science topics. A doctorate in the social sciences tends to make an academic not a field research worker.

There are also other important factors to consider which do not apply to research in developed societies. Domestic research funding is to all extent and purposes non-existent and in this my work which was nearly always without any funding at all must parallel the contemporary situation and therefore methodically one has to do with local fundless realities and cheap methodologies.

There are therefore very serious and cogent reasons for using methods which are cheap and short and which can be used as a by-product of other activities for which researchers are being maintained. Rockefeller Foundation had a Gap Project for secondary students wanting to study law as the end of the school year and the start of the university term were six months apart. I used three of these young men on a variety of projects in Mombasa relating to the distribution of court cases, housing shortages and the geographical distribution of various types of prostitution.

Thus research is often related to the vacations of university students and work of employees in hospitals and schools; wherever they are and whatever they do it is likely to be original work and since they are working on what they would be doing anyway, it is costless, as with a hospital assistant in Nassa working out the numbers of patients treated who did not complete their courses of treatment, which was over 40%.

Inevitably I became separated from what might be called open air field work. Firstly I became older and less inclined to be involved in the discomforts of field work. Secondly political change made it reasonable rather than necessary to leave East Africa and this could have led to a concentration on theoretical development of my data. However I found that the thirty year break in field work led to valuable comparisons allied to the work of recent researchers (Tanner and

Wijzen, 1993; Tanner, 1995).

In addition these twenty years of field work led to two major works on the biological rather than psychological aspects of religion (Reynolds and Tanner, 1983, 1995) as well as to as number of other studies (Tanner, 1993, 1994).

6. THE PROBLEM OF THE 'OTHER' AS A SELF-INFLICTED HANDICAP

If I had come into the field work situation from another social and cultural world, I would have been faced and forced into a precipitous 'us' and 'them' situations. On the other hand I lived in what was in effect a permanent field work situation and the 'Other' in this situation could not be distinguished clearly from my personal domestic arrangements.

It was not so much that the 'Other' started at the end of the garden but they were probably inside the house as well; friends, servants, associates and indeed my wife and son. The 'Other' in this situation was everyone else, not just the objects of research.

So for me the 'Other' started in his own house. Thus I was at least a part, if not always a part of the 'Other' (Tanner, 1963, 1964). In other situations as a non-participant observer I was still part, if only a small and possibly interfering part of the 'Other' (Tanner, 1955).

The denial of coevalness which comes from an overdelineated concept of the 'Other' is surely the result of the 'Otherness' of the field workers themselves rather than of the objects of research. The Burma Frontier Service in which I had worked initially had a number of officers married to local ladies and this combined with the fact that the annual reports on my conduct were written by locally born senior officers inhibited any burgeoning of the concept of the 'Other' and with this the impossibility of coevalness. This was apart from the fact of constant touring in Burma during which my wife and I talked, drank, danced and worked with the people amongst whom we were living in almost total isolation from European influence other than firearms.

Thus on my transfer to Tanganyika I was not prepared to fit in with the political and social concept of the 'Other' with whom it was not possible to be coeval. Possibly in this my wife and I were years ahead of European thought and practice in East Africa and thus became the 'Other' in many situations to the European 'Us'. Although I only realised this in retro-

spect, it certainly made field work considerably easier as accidental social outsiders it became a space filler for the time which otherwise for a person in my position would have been spent in quasi-obligatory socialising with other Europeans.

I never worried about the 'Other' except in the sense of a defined and almost self-limiting objective in which the specific 'Other' lasted as long as the research project, over which there could not be any moral dilemma as to who I was and what I was doing. My overview of East African drug addiction (Tanner, 1966) was no different to any comparable study in Europe and America in which there are few and/or similar moral dilemmas for the researcher. Researchers such as myself want to describe certain behaviour patterns. If the researcher wants to propose new and innovative ideas in the style of O'Leary, the problem of the 'Other' then is the social and political structure of that society, rather than the objects of that research. This perhaps should be no concern to the researcher but led in East Africa at that time to initially social isolation from the European community of which I was officially a part and subsequently to isolation within a university community isolated from the surrounding social and political structure.

If a piece of research involves a worrying concept of the 'Other' why do it at all? It is not just some part of post-colonial or class guilt that creates a problem of the 'Other' but a whole range of research topics involving a distinction between the so-called straight and the so-called deviant.

If I had not foreseen that research into homosexuality for the duration of a sentence of imprisonment was an essential part of understanding convict behaviour, then indeed it would have been a problem (Tanner, 1966). Perhaps one needs a developed sense of moral neutrality which comes with serious inquisitiveness.

Even latter when I came to attempt comparative research into religious behaviour, I was repeatedly forced to conclude that whoever the 'Other' was, it was by no means clear where if anywhere was the dividing line between myself and the range of 'Others'. I was clearly a part of the Roman Catholic 'Other' when I researched the reasons for joining and living that Church in Usukuma (Tanner, 1968 a, b, 1969) in contract operational research into Roman Catholic behaviour in Britain, Zimbabwe, Guyana and Zambia.

I was less of another in researching ecumenism

in East Africa (Tanner, 1973) and in contract research into the use of social development money by Christian agencies in East Africa because I was no longer living in East Africa. Certainly research can be seen as a job like any other work whether it is paid for under contract or under a self-imposed target related to my own income and available time.

I have always considered the behaviour of anyone other than myself to be quite extraordinary. I was emeshed in 'Otherness' whether it was European 'tribal' or Tanganyikan 'tribal' customs. It never struck me that they were so different that the former were 'Us' and the Tanganyikans 'Others'. This struck me then as it still does as ethnocentrism or at least as cultural superiority rationalised. As I remarked once in addressing a British school when I was asked if I had seen any strange ceremonies in Africa, nothing to match the strangeness of European competitive formation ballroom dancing.

It has also struck me that a preoccupation with the 'Other' was probably the result of too little field work or more specifically no field work experience in their own cultures and too much intellectualism in a detached academic environment. As it might be paraphrased 'if you can do fieldwork, you do it; if you cannot, you teach'.

7. OBJECTS AND OBJECTIVES OF RESEARCH

What did the writer research and why? It seemed to me that all around me were knowledge blanks into any of which I could go with interest. Since I was not by predefinition a specialist in anything, it did not seem to me that I had to do anything in particular. Marshall Ney when asked what was his theory of war replied that he rode to the sound of the bugles; the writer seemed to have done the same thing. Thus I now discuss some of the research projects which I carried out and to detail the methodologies used.

A. Methodologies Related to Systems Theories

Even in the most underdeveloped areas there are always lists from which it was possible to start research even if one assumes that they are never accurate, since I cannot have been the only one to do annual administrative statistics with a pencil and rubber, which led to an interest in Third World statistics relating to law and order (Tanner, 1983).

Local court records show what is not the subject of public dispute as much as what is. There are always tax lists which should mirror the age pyramid of the male population but may in fact represent social standing or social inferiority. Ferries on key roads recorded the flow of migrants in and out of Ngara district to and from Uganda. The stocks of local traders in Ngara as against local sales showed the extent of cross-border smuggling of patent medicines into Ruanda where they were prohibited. The Traffic prosecutions in Mombasa showed that where the magistrates and offenders belonged to different ethnic groups, Indians got the heaviest fines than either Europeans or Africans. Fluctuations in traffic offences were correlated to typist availability. No part of any state anywhere functions without attempting lists, records of what has happened and they are not usually deliberately inaccurate.

These systems research projects often came from the fact that the data was just sitting there waiting to be collected or data could be collected with very little effort; they could be called 'lollipop' projects.

1. While acting as Tutor for Extra-Mural Studies in Mombasa I came across a retired Afro-Arab Hospital Assistant who had lived there from birth. From previous work on the East African coast I was aware of the preferred cousin marriages of Moslems. Over a year this man wrote down the family trees of every Afro-Arab known to him who lived or had lived in Mombasa. From this I was able to work out the percentages of single and double cousin marriages which had occurred (Tanner, 1964). The only difficulty in this was the informant's cultural difficulty in recognising the biological importance of the female line.
2. In the course of this study the writer visited the Mombasa Maternity Clinic and found that the records had been kept by the same Sister Midwife in her own handwriting for seventeen years, for some ten thousand births with demographic and medical details. I assumed in these circumstances that there would be standard errors and as such these records would be unusual in consistency and reliability and thus was valuable data (Roberts and Tanner, 1963). Most hospitals and clinics have such records.
3. Roman Catholic churches globally keep standard records. The records of Ngara churches for infants baptised in anticipa-

tion of their deaths showed the frequency of twins rather than birth rates in general. The parents of twins baptised in these circumstances would thus be able to avoid expensive customary rituals. These records also show the size of families and the dates of their rites of passage. Such a standard system allows for the collection of wide-ranging comparative data.

4. I was sitting in his office in Kampala when I heard rifle fire from the town. I got four students (different sexes and tribes) to collect rumours from coffee shops and bus queues (Tanner, 1978).
5. In turning my tutoring in the Mombasa Prison into the background for a research project into prison behaviour, the long period before I was accepted by the convicts as not working for the police, was spent going through the prison records of visits and by whom, prosecutions for breaking prison rules and which warder prosecuted, weights of convicts and other voluminous records. The prison store was full of records.
6. The sisal estates in Pangani on the Tanganyika coast had details of what tasks the workers did and other employment details. It was possible to work out productivity and workers needs since some were able to complete three tasks in a day although one task was regarded as normal allowing the day's work to be finished before noon.
7. After giving some talks to a community of teaching nuns, I asked them to get their girls to write a personal prayer. A word analysis of these prayers showed that after puberty prayers became more personal than familial; the word for husband and God was the same.
8. The boys of the middle school near to Pangani township were asked to list what they liked and disliked about the behaviour of Africans, Europeans and Indians. The results showed the dominance of ethnic fictions over facts started at an early age.
9. In Musoma cattle theft was rife but little was reported to the police. I did a village by village listing of thefts over a year plotting on a map from where they had been stolen to where the tracks had been finally lost. This showed that theft was an internal problem and that contrary to tribal opinion outsiders were not involved and that it might be related to a shortage of animal protein.

- Further that the total head stolen in that past year was over twenty thousand and not the two hundred head reported to the police (Tanner, 1966).
10. I found it possible to get reasonable information on population movements by asking basically from which direction people had come rather than asking politically tricky questions about tribe and getting inaccurate answers to questions about age (Tanner, 1961). This method avoided the cumbersome and inaccurate method of working from ages and dates.
 11. On the other hand systems methods may in fact collapse under the weight of the material collected. I was asked to do the Tanganyikan part of a Ford Foundation six nation study of missionary education (Morgan and Tanner, 1976). I had hoped to reduce each answer in the questionnaire to ten types of replies. On one question alone I identified over three hundred different types of answers from the 5000 replies correlated to basic demographic differences which led me to be sceptical about the use of questionnaires even in the most simplistic type of 'market' research. I found that there was a permanent impulse among researchers to use questionnaires even if they suspected that the data therein was not good simply because under the conditions of slow moving field work, the pile of paper was at least a sign of something achieved. My attic still contains numbers of administered but somewhat suspect questionnaires.
 12. However a very easy and useful form of the systematic collection of data was using single key words and asking almost anybody who happened to be at hand to make up sentences using those words. Literally one morning in a school could and did provide the pragmatic basis on which ideas could subsequently be developed; write down ten sentences using the word 'God'. This provided me with the basic ideas of the Sukuma religion and subsequently data on law and order, sexism etc in Uganda.
 13. The analysis of voting behaviour is always tricky and unreliable because it is secret but there is no other data available. In an election dominated by a single party what sort of person gets elected and a study showed that bus-conductors were completely successful (Tanner, 1962) because they were known to many people.
 14. In a society which was almost completely Christian, it seemed useful to assess to what extent Protestants and Roman Catholics were still using traditional religious protections. Using cross sex and cross faith helpers who visited bathing places, it was possible to find out the proportions wearing amulets as opposed to religious medals under their clothes (Tanner, 1975).
- ### B. Methodologies Related to Action Theories
1. After leaving East Africa I became Lecturer in Sociology at the Jesuit seminary of Heythrop before it became a constituent College within London University. I was asked to evaluate the work of the Jesuits in Zambia, Zimbabwe and Guyana for which six weeks in each location was allowed. These trips involved free-form taped interviews with any priest, nun or brother who consented to be interviewed and exhausting travel. I asked only one question 'what do you do', and the length of these interviews varied between a twenty minute stand-off to a total of fourteen hours over several days with one priest. These resulted in a series of reports in the form of important issues and possible solutions. No statistics were involved and more than two-thirds of religious personnel in each country were interviewed. It was found impossible to analyse these taped interviews not so much because of their length but because of the number of points raised and their unequal variety. They were in fact never listened to again, and indeed presumably remain of historical interest. I wrote the reports up from my notes taken during these interviews and the accumulating sense of what was happening. There is also an important security angle in taping if the privacy of informants is to be respected and the information gathered is of a sensitive nature relating to crime and politics. My tapes in Zambia were stolen possibly by their security service and fortunately my coded notes on adultery in the Tanganyika civil service were stolen with my clothes while I was bathing in the sea.
 2. In my prison research interviews became important to the convicts in a dull and routinised existence. To act as informants

they had to be interesting and the contents of these taped interviews could be a check on their consistency. These sessions were regarded as entertainment by informants for which the only material reward was occasional cigarettes.

Literate informants kept diaries on lavatory paper recording any occasions in which anyone shouted at anyone else, as well as their life histories. A large quantity of material was produced but in retrospect much was repetitive and unnecessary if more time had been spent in preliminary analyses.

3. While working with the Sukuma I got my helpers to write essays on topics for which I put key vernacular words on the tops of sheets of paper. This produced usable and idiomatic quotes when writing up the significance of these key words. The fact that there was a key word on each piece of paper made for easy filing and coordinating. Again this produced much more material than I was able to handle or possibly for anyone to handle and in the main they remain unread.
4. Translation and its preparatory work is action masquerading as systems methodology. The collection of interesting phrases and words is a fascinating time filler for rest periods, marching and fireside conversations. The importance of this background updating for bilingualism became apparent when translating a training manual for the International Labour Office (Tanner, 1960) as I found that there were no vernacular texts for use in the teaching of industrial relations, and a joint translation with F. Kamoga of Macchiavelli's *The Prince* (Tanner, 1968), when I found that there were no vernacular texts for the teaching of political science, other than a Swahili translation of the Communist Manifesto.
5. Later I became interested in mistranslations between the Swahili penal code and the basic English text (Tanner, 1966) and in the Bible Society's translation of the New Testament with the constant problem of the literary versus the commonplace use of language (Tanner, 1979). I then became aware that translation not only often involved a choice of words but these choices reflected moral more than linguistic alternatives (Tanner, 1993).

10. CONCLUSIONS

What can be concluded from all these and many other pieces of research which reached an adequate enough standard to be published.

1. Material collected, even if the researchers cannot use it themselves, can be useful to someone somewhere sometime. It possibly pays to be generous and give it away rather than to sit on it. If it is not written up within a year, it will never be written up. I have given away much research data on demography and still have a lot which I should give away.
2. Any research can have interesting spin offs into other fields. I enjoyed working for other people without charge rather than for myself alone and the contacts they provided and other specialists got the benefit. In this respect it perhaps pays not to be ambitious, and not to jealously guard research material.
3. In the sense of producing results which can have some immediate influence in developing societies rather than being part of some theoretical development, most research takes far too long. Most of my research in terms of work hours rather than calendar periods perhaps took three months.
4. Unsubsidised researchers are a better investment for they have more to gain by completion than subsidised researchers who already have their feet in the academic door. Lack of money for research leads to inventiveness and some sense of urgency.
5. The wider the range of one's research or conversely the less that the boundaries of disciplines are observed, the greater the accumulation of data that can be pulled out later for uses that could not possibly be foreseen. It pays professionally and personally to be a polymath in an age of specialisation. You may not eat in the halls of the mighty but you may have more fun and keep usefully occupied. Funds and opportunities are always scarce; to specialise is to limit these opportunities. If one is alert enough to take advantage of any opportunity not outside one's capabilities, then one is better off all round.
6. This high degree of productivity and research involvement has costs which are both professional and personal. Time spent in one area, means time lost in another. Pro-

professional progress in anthropology as in business is a cliquey affair; it is not what you know but who you know which is important. The more time that is spent in this type of academic socialising, the less is available for research.

7. Personal relations with informants and assistants are satisfying and inevitably short term. Personal friendships and the understanding of wives, husbands and children may be sacrificed and they would have been of longer term benefit. Any researcher for the longer run of shared life must realise that these count for more at the end of the day and pay a profounder and more satisfying dividend.
8. Even agreeing with Geertz's view (Geertz, 1978: 44) that British and American anthropologists were not too comfortable with symbolic absolutes, I took it as axiomatic that if I could not explain an idea in KiSwahili to a Swahili speaker, the fault was either mine or the idea did not have a workable existence in that culture.
Perhaps there is some truth that theoretical abstractions in social research are in themselves distancing from any understanding of those cultures to which they are applied. The Sukuma understood familial segmentation quickly enough and that spirit possession was at least one way in which women could regain lost influence. However to explain traditional healers as having the characteristics of schizophrenics (Field, 1960) is to introduce western concepts into Sukuma life which has as much validity as suggesting that westerners become schizophrenics in part because they do not believe in ancestor spirits possessing people.
9. The necessity of bilingualism and many classic works would founder on this account alone if the linguistic abilities of their writers were in fact to be tested. As one's understanding of another language increases, ethnocentrism must diminish and one's appreciation of the mental complexities of the other culture increase. At the same time the people in these other cultures become less and less homogeneous and one attracts as informants people of an equivalent intellect to oneself. My *Transition in African belief* (Tanner, 1969) covering the religious behaviour of three million people was based on the religious behaviour of perhaps twelve

Sukuma with whom I happened to have formed a relationship by accident.

10. Bilingualism has to be used in the context of the other language in which one is bilingual. Evans-Pritchard's work on the Azande was translatable into words that had meaning for Swahili speakers. Levi-Bruhl's *Tristes Tropiques* is not only difficult to understand but impossible to put into Swahili which is a complex and wide ranging language; it was perhaps written for the writer's own benefit and for an inner circle of cogniscenti. The people in other cultures are not stupid if they cannot understand what you are trying to say; their minds are perhaps working quicker than yours.
11. Finally whatever the methodology, the enemy are not editors, academic supervisors or obdurate informants. No one comes this way again and what is seen and recorded or indeed missed today has gone into history. The enemy is the passage of time. Researchers must have a sense of immediacy and be pushed on by the wonder and excitement of human diversity.

NOTE

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KEY WORDS Opportunity. Luck. Training. Enthusiasm. Bilingualism

ABSTRACT Over twenty years of field work, the importance is shown of data controlling the methodology. The inadvisability of specialisation if advantage is to be taken of research opportunities. The need for sharing data and working with as well as for others is stressed. The basic need for bilingualism in all cross-cultural research.

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